



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

SCIENCE

FRIDAY, JUNE 26, 1914

THE TREND AND INFLUENCE OF CERTAIN PHASES OF TAXONOMY¹

CONTENTS

<i>The Trend and Influence of Certain Phases of Taxonomy:</i> PROFESSOR AVEN NELSON ..	921
<i>An Experimental Determination of the Earth's Elastic Properties:</i> PROFESSOR HENRY G. GALE	927
<i>Eduard Suess:</i> PROFESSOR CHAS. SCHUCHERT.	933
<i>Scientific Notes and News</i>	935
<i>University and Educational News</i>	939
<i>Discussion and Correspondence:—</i>	
<i>Dadourian's Analytical Mechanics:</i> PROFESSOR E. W. RETTGER. <i>Accessory Chromosomes in Man:</i> PROFESSOR M. F. GUYER	940
<i>Scientific Books:—</i>	
<i>Smith's Chemistry in America:</i> DR. IRA REMSEN. <i>Das Relativitätsprinzip:</i> PROFESSOR E. B. WILSON. <i>Nottram on Controlled Natural Selection:</i> PROFESSOR W. E. CASTLE	942
<i>The American Ephemeris and Nautical Almanac for 1916:</i> J. A. HOOGEWERFF	945
<i>Botanical Notes:—</i>	
<i>Forest Tree Diseases; Another Tree Book; A Pharmaceutical Botany; Flora of South-eastern Washington; More Florida Manuals; Short Notes:</i> PROFESSOR CHARLES E. BESSEY	946
<i>Special Articles:—</i>	
<i>Cell Permeability for Acids:</i> PROFESSOR E. NEWTON HARVEY. <i>A Destructive Strawberry Disease:</i> PROFESSOR F. L. STEVENS	947
<i>The American Chemical Society:</i> DR. CHARLES L. PARSONS	950

"ENOUGH is sometimes too much," says the newspaper philosopher. I suspect some of you are thinking right now that we have already had enough systematic botany, but as briefly as I can I wish to try to show you that while it is true that we have already had too much, it is equally true that we have had too little.

Do not feel alarmed because of the magnitude of my subject. I shall not deal with it as a whole—only certain phases of it and their influence. Before attempting my main message may I voice a plea for the old-time systematic botany? It is of course primarily the handmaid to all of the other subdivisions of the science, but apart from that is it not in itself a desideratum of no small moment?

It trains the perceptive faculties, teaches orderliness, develops judgment and strengthens reason. It is therefore a cultural course of no small significance to all who take it and, as some of us know, the source of much pleasure to many. There is a saving grace in botany not found in most of the other sciences and this is exercised through taxonomy more fully than through all its other divisions combined. Systematic work for its own sake is distinctly worth while. It develops in the student or the amateur, who achieves a fair measure of success, a feeling of confidence in himself and gives that stimulus for further mental effort that only the conquering of a definite problem affords. In this respect it may be compared to mathe-

MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

¹ Read before the Botanical Society of America at Atlanta, December 30, 1913.

matics, with the added advantage that the flavor of the pleasure derived recurs again and again as the fields and woods minister to his life, and spring, summer and autumn, yes, and even winter, in turn speak to him who understands their glad greetings of the passing years.

Let no one imagine that it is merely easy recreation for the *dilettante* in science. It is a man's job. Any one who succeeds in systematic work would measure up well in the philosophical subjects. Manuals and keys can be made only for those who can read as much between the lines as in them; those in whom the power of discrimination becomes strongly developed but who ease up its severity by the due exercise of judgment and reason.

Systematic botany furnishes to the average layman, who is scientifically inclined, a more continuous incentive for pleasurable and inspiring contact with the world about him than any other subject that claims to be worthy of his attention. It may be that it represents the primitive phase of our development, but does not all development begin with the primitive? That some never get beyond the primitive stage is neither here nor there. The same would be found true in any other subject whatsoever. I raise the question if it is not largely true that the best botanists we have or have had began their career as systematists? Were they not led into the subject by this door? Their love for plants, their desire to know them, determined their careers. We may be evolving greater and greater men in the science, but even these must of necessity touch at least the high points in the road by which the race of botanists have attained the crowning glory of the present. The recapitulation theory is as universally applicable as the theory of evolution itself.

Let us look a little farther into the careers of those whose names have come down

from a former generation. To save time we will take a single example, one who was not only a systematist, but the peer of any in his generation in every other line. His name is known to more people in America, even a quarter of a century after his death, than that of any other botanist of any time or place. His bust found its way into the "Hall of Fame" because he did more than any one else to make it possible for people to know plants. He was admired and loved in his day and now because of his "Manual" and the accompanying "Lessons." Let it not be forgotten that he would still have been a distinguished botanist had he given no thought to systematic work. His grasp of structural and physiological problems was far in advance of his time, and who knows whether even his philosophy may not prove to have been more profound than some of his critics will now admit? Dr. Gray found his way into the hearts of the people and enriched their lives by opening for them a larger world than would otherwise have been possible to them.

It is true that in all the botanical fields there are great outstanding characters whom we do not ordinarily think of as systematists. These are, however, men or women who have rendered some signal service to the race by promoting its physical or economic welfare, but even these did much of systematic work before they were able to share with others the results of their achievements. Again, to take but one example, we have in Pasteur a name that will live so long as living things are subject to attack from microorganisms. He made the race his debtor, not only by what he himself achieved in bacteriology, but by opening the way into the new field. The work of his disciples in preventing and alleviating suffering in man and beast must also in part be accounted unto him for right-

eousness. Such men, however, were entering new fields and had to create descriptions and systems of classification as a foundation for their work and as a medium of communication with their fellows. Thus we have come back to the original statement, systematic botany is primarily the handmaid to all the other subdivisions of the subject.

Having said this much in commendation of taxonomy in general, kindly permit now a brief consideration of its trend and influence. If taxonomy and taxonomists are gaining in prestige and power, if the other departments of botany are each year being better served, if the average layman in the field finds it easier to know the plants themselves we may congratulate ourselves and say that all is well. If the reverse is true, something is radically wrong. A cancer is eating its way into a vital part of the body of our science.

Taxonomists were never so numerous nor more active than now. But all activity is not necessarily progress. Motion up and down may be spectacular and nothing more. Never were there so many devoting themselves to this subject professionally as at present. Literature is piling up volume upon volume. Before we can determine whether this is progress or recession we shall have to try to find the purpose of it all. The description and classification of plants is not in itself an end. It is a means to an end and that end not for the specialist himself, but rather for his colleagues in other lines and for the great army of intelligent men and women who love plants for their own sakes.

The reasons why people may wish to know plants are many, most of them entirely worthy. No reason is more legitimate than the mere desire to know that is almost universal until our method of education, or lack of method, kills the de-

sire. Desire that is never satisfied dies afterwhile. The child asks, "What is it?" but when it has received the answer, "I don't know; stop bothering me," seventy times seven its interest either wholly dies or it seeks outlet in other channels. The furore of enthusiasm about nature study I fear has largely spent itself. The best statement of the purpose of nature study that came to me was "It aims to keep alive the child's tentacles of inquiry." Are we not largely failing in the attainment of this meritorious aim? If so, why? As I know our schools it is primarily because of the lamentable ignorance of all nature subjects by the teachers. Not only by teachers in general, but by those professing to teach botany in our high schools. A large majority of them wouldn't know an elm from a holly or an evening primrose from a lily. I have seen them by the score in my state and most of them came from outside schools of high standing where they had been trained in the cytology of plants that they never saw and in the ecology of plants that were left behind in the dreams and environment of yesterday. You may wonder how this relates itself to my subject. But listen! There is no reason for the existence of the professional systematist (apart from the growth and pleasure it yields him personally) unless his efforts produce results that make it more easily possible for others to know the plants in which they become interested. If he fails in this one thing he fails in all. May we not judge by the indifference of the multitude to our work; by the hopelessness of the amateur who tries to acquaint himself with the plants of his district; by the distrust of their results by even professed systematists, and by the none too well concealed cynicism of our colleagues in other lines, that we are failing in this? There seems to be nearly universal agreement

that it has become increasingly difficult for every one, for any one, to state with any degree of definiteness the correct name of any considerable number of plants. That we are in a muddle is evident. That we shall never be able to clear it up I do not believe. I shall not pretend, however, that I am wise enough to tell you how this is to be done. I very much doubt if any one knows at present just what to do next, but at least no harm can come from a free discussion. If we but knew just what has gotten us into our present plight it would simplify matters, but even then the application of the remedy would be difficult. We have each so long been a law unto ourselves that it will be impossible to secure any considerable unity of action at once. Particularly will this be true if there is no agreement that a remedy is needed. Some will feel so, in spite of the fact that a large majority of the botanists of this country would subscribe to the following arraignment: Our work has been analytic, not constructive. We have dismembered organisms and held up to view their component parts. We have been looking for differences and with such amazing success that the fundamental resemblances have largely escaped our notice. We have thus produced a *pot-pourri* that is the despair of every one except ourselves, and most of us do not know how to unravel our own mysteries.

I know this is a terrible charge to lay at our own doors, but perhaps it comes with better grace from one whom others have chosen to consider as *particeps criminis*. I dare not flatter myself that I have been even one of the chief offenders, but I acknowledge with humiliation that I have had a small share in producing the disaster that has overtaken us. I now stand before you thoroughly repentant. Would that I, like the reformed inebriate or the reclaimed

sinner, could preach a gospel of reform with such fiery zeal that I should reach my erring brothers.

I know that only the dead make no mistakes. We have been passing through a period of great botanical activity and he who has not made many mistakes is not much of a botanist. It is better to have been alive for a decade and have to face our errors than to have been lying immobile blankly gazing at the stars for a millenium. However, there is no virtue in mistakes as such. Our endeavor should be the maximum of activity and progress with a minimum of error and lost motion.

That the names of plants have become so unnecessarily burdened with synonyms may be partly accounted for by the following considerations.

1. We have been so busy looking for differences that we have forgotten that classification is fundamentally based upon resemblances. A distinguished systematist has said that there are two kinds of botanists—"those who see differences and those who do not." I fear that some of the former class have had their discriminating faculties over-stimulated, since species have been founded upon and keyed out upon such valueless characters as one fourth mm. in the length of the stigma and scores of others even less evident.

2. We have thrown down the old concept of a species and we find ourselves in a jungle of illy defined forms out of which we shall never be able to come until we are willing to chop out the water sprouts that grow among and often from the loftier trees. Time tests many species. It is not conclusive, but it is very presumptive evidence against their validity if, as years pass and further collections are made, no other specimens are referred to them. In examining the material in any large herbarium one finds many such hermit sheets.

Let me suggest that there are also two kinds of species, those that exist more or less well defined in nature and those that have only an academic standing. Into which category the different ones will ultimately fall is not in the power of any one mind to settle, for we recognize the truth as expressed by Dr. Gray when he said:

Species . . . are not facts or things, but judgments, and of course fallible judgments; how fallible the working naturalist knows and feels more than any one else.

We often hear of "critical species" and arguments are multiplied to defend their retention in literature. Surely it is true that some of them are valid and stronger even when held on avowedly technical characters than some of the supposedly evident ones that have long been accepted. Nevertheless, one can not help suspecting that the condition of many of these is so "critical" that they can not long survive the untoward conditions that a general upheaval in systematic botany will superinduce.

3. Some of the synonyms are the direct result of mistakes other than that of drawing overfine distinctions. To enumerate the countless causes for these errors is neither desirable nor possible. For each there is always an explanation, not necessarily an excuse. As already stated, error is inseparable from activity. Legislation that would limit publication to those having experience and who are working in a proper environment would be desirable but for two things: (a) It would cut off the future supply of systematists and (b) it is impossible of enforcement. Since prohibition is scarcely possible and surely not desirable, regulation might be attempted. Seriously, why should any one publish a species in a genus in which the known indigenous ones are not all clear to him, unless it be in a genus separable into strongly

marked sections. In that case one might work with some assurance of certainty if all the species in the section were known.

4. It sometimes proves disastrous to assume, as is often done, an inherent improbability that the same species will not be found in districts widely separated geographically. Environmental factors must be reckoned with and these have a trick of repeating themselves in far distant and most unexpected places. Mistakes would be enormously reduced if every one was expected to definitely locate the proposed species in the genus, keying out the species if necessary, or only those of the section should its sectional relationship be apparent.

It is one thing to describe a plant and say (as I and others have done) "apparently not very near any of the hitherto known species." It is quite another to so describe it that it shall be properly contrasted with its nearest ally and its setting in the genus made evident.

It is always hazardous to publish in a large genus unless the examination of its content amounts practically to a tentative monograph. Take a genus at random, *Arnica* for instance, and even a superficial examination of the material in any large herbarium will reveal a number of good species each of which has been characterized by several during the last two decades, apparently because each felt free to assume, for instance, that Colorado and Washington were, for phytographical purposes, on different planets.

5. Another source of error lies in our adherence to different codes or to no codes at all. International law and comity are swept aside. Lawlessness always did mean anarchy in political and social life, and it has brought the same result in taxonomic nomenclature. The moral is not hard to find.

6. Our strength has not all been used in the promotion of constructive work. We spend too much time in criticizing the work of others or defending our own species. Naturally our own children are much better than others, but I doubt if we gain much by rushing to their defense whenever they are attacked. This *species-making* is merely for a day; *species characterization* is for all time. It is true that they may be thrown down to-day and erected to-morrow, but in the course of time the worthy will be established and the worthless will go to synonymy. To love our own is well, but to love them so well as to be willing to juggle the testimony is vicious. Pages and pages are wasted in criticism, recrimination and the imputing of wrong motives. The inexperienced alone are convinced by such speciousness. Those who have learned wisdom know that the attacked party, were he so minded, could put up an equally effective defense. Is it not better, however, to use all available time in productive work, knowing that nothing gets its final rating until established or disestablished by critical monographic work. The one thing we can not afford to be guilty of is insincerity. We simply must deal honestly with nature and justly with the work of our fellows. Personally I would rather my whole brood should perish than to save even the most promising by dissimulation or misrepresentation.

But I must not carry the inquiry as to causes further. There are many questions I had intended to raise, but time will not permit. I must condense into a few paragraphs just a thought as to the influence of the chaotic condition of taxonomy upon the progress of our science as a whole. Morphology, physiology, ecology and economic botany in its scores of applications have all gone forward by leaps and bounds, but it is (dare I say it?) in spite of, not

by the aid of taxonomy. Our unstable nomenclature, involved synonymy, multitudinous, often "half-baked" species have produced the conditions described in this paper. The effect must of necessity be to retard, to discourage, to divert effort.

Now lest I be misunderstood let me say that taxonomic work has not all been misdirected—far from it. Keeness of observation and great powers of discrimination are noteworthy in the work done. It is not so much that what has been done should not have been done, but rather that much greater effort ought to have been made to relate recent work to that which had gone before. Synthesis should have followed so closely upon the analysis of the elements of our flora that duplications would promptly have been discovered and the relation of each element to the other detected and stated.

If we will keep in mind that technical systematic work does not exist primarily for its own sake; that when it ceases to be a means of culture and pleasure to others; that when it becomes burdensome to and unworkable by our fellow botanists in other lines—the chief reasons for its existence have passed, then we shall see more clearly what yet remains to be done. We need to popularize our subject, but not by writing down to those who know little and care less, but by classifying our work so that those who wish to know shall be able to understand. We need more local descriptive floras with well-made keys and illustrations. Our manuals have become too bulky; we cover so much territory that the species are necessarily very numerous. The more species there are in a given genus the more complicated the key and the slighter the differences that separate the species. We ought to have many handbooks and pocket manuals such as the one

Professor and Mrs. H. M. Hall have given us of the Yosemite.

We have had a feeling that our manuals must cover vast sections of the country, many hundreds of thousands of square miles; that they must be complete, accounting for everything ever mentioned. As a result much is found in our volumes that describes things that do not exist, are very rare or have only historical interest for the technically trained. I am pleading for those who want to *know* the plants that relate themselves to their professional work, to their mental life or to their recreations. Please note I said know the *plants*, not know *plant names*. No one wishes to know names apart from the plants in which he is interested. Knowing the plant is first, and then a name becomes indispensable.

And why not a name instead of a manufactured phrase palmed off as an English name? In what respect is "purple-stemmed swamp beggar ticks" better than the name? We use geranium, magnolia, forsythia, and scores of others. Why not phlox, mertensia, chrysopsis or practically any other generic name? It is true this only designates the genus, but this is all that many who are intensely interested in the plants care to know, as exemplified by our use of the words clematis, chrysanthemum, lupines and roses. Those who wish to designate the species can do so with more celerity and certainty by saying *Phlox glabrata* than by smooth-leaved sweet william. In my recent "Spring Flora" I proposed this use of the generic name seriously and I wish to assert that I have seen no reason for changing my opinion.

In closing let me express the belief that we are on the eve of a new era. Already the pendulum is swinging back. The dismemberment of genera and the multiplications of species proceed more cautiously. New species will continue to be found even

in this country (hundreds of them). These ought to be and will be published. So long as work is done errors must occur, but the percentage of error, let us hope, will be greatly reduced, while the disturbing effect will be minimized by more and more of constructive work of the compendium type.

AVEN NELSON

UNIVERSITY OF WYOMING,
LARAMIE, WYO.

ON AN EXPERIMENTAL DETERMINATION
OF THE EARTH'S ELASTIC
PROPERTIES

It is well known that the ocean tides are caused by the differences in the attraction of the sun and moon for the surface and center of the earth. These differential forces are very small compared with the attraction of the earth for bodies on its surface; in round numbers the joint tidal force of the sun and moon on a body at the earth's surface under the most favorable circumstances amounts to only about 1/10,000,000 of the weight of the body. This force would deflect the bob of a plumb line 10 feet long from its normal position only about 1/100,000 of an inch. This deviation corresponds to an angle of only .02", or the angle which the head of a pin would subtend at a distance of 10 miles.

If the earth were a perfectly fluid mass, *i. e.*, if it offered no resistance, either elastic or viscous, to changes of shape, the surface would be tilted by the tidal forces through this same angle, and the new horizontal would be perpendicular to the new vertical. There would therefore be no change of the plumb-line relative to the earth's surface, and we could not detect the so-called "deflection of the vertical."

If the earth were perfectly rigid the plumb line would move back and forth, as the positions of the sun and moon vary, by an amount which can be calculated with an accuracy which is limited only by our knowledge of the masses and relative positions of the sun, earth and moon. As a matter of fact, the earth is partially and not entirely rigid, and therefore the excursions of the plumb line are a certain